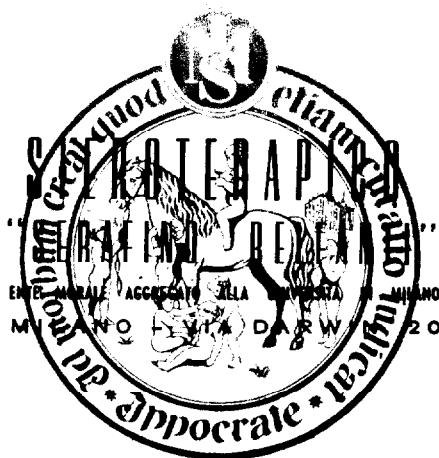


ISTITUTO



MILANESE

3/4/52

Dear Lederberg,

Thank you for your letter of March 26, which reached me two days ago. I have sent a letter to Hayes, of which I enclose a copy, because he might already have sent his paper + or ~~be likely to~~ send it very soon to the editors of JGM, and on further delay publication on the same issue ~~may~~^{might} become impossible. Thank you for your news re origin of resistance; I hope the reprint will reach me in time.

* preceding one was TLB₁-S⁺sugars - x TLB₁+sugars +
The problem of F⁺ effect on segregation is fascinating, but rather difficult to explain on current hypotheses. I have tested ~~that~~ asymmetry of segregations, analogous to that I described in my earlier letter, also in the reversed crosses (i.e. * TLB₁-S⁺sugars + x TLB₁+sugars -, F⁺ x F⁻ and F⁻ x F⁺) ; it is almost superimposable on the preceding one. F⁺ x F⁺ crosses are somewhat intermediate, occasionally with some bias in one or other sense (which may be in agreement with your scheme of relative sexuality) i.e. resembling more one ~~than~~ the other of the two corresponding F⁺ x F⁻, F⁻ x F⁺ crosses. Asymmetry is also found in BM- x W 945, F⁺ x F⁻ and F⁻ x F⁺ ; F⁺ x F⁺ intermediate. I have found myself testing mentally the wildest hypotheses. It may be that the F⁺ parent contributes a "shorter" chromosome - but I am now favoring the idea that the F⁺ "gamete" carries a single strand, while the F⁻ gamete carries more than one (polytenic or multinuclear ?) and that crossing-over can happen repeatedly before segregation. However, even if the system behaved as a multivalent, with a single round of crossing-over, it might explain the elimination of the contribution from the F⁺ parent, subject to the restrictions due to the markers. You are in a much better position with Het, where no fixed markers need being employed. Could the data obtained from the Het segregations be explained assuming that fertilization results from the union of a haploid (F⁺) gamete with a polyploid (F⁻) one, ~~and that a multivalent is formed.~~

I have no new data on Hfr; I have never ~~kept~~^{tested} attenuated strains, but shall do so. I shall test more crosses Hfr x x TLB₁-F⁺ to see if the differences I have found in behaviour reappear, and send you the relevant cultures.

Re Mrs. Lederberg's question on NCTC 123 : it was possible to grow 123 on minimal + methionine + lysine, and thus select a few auxotrophs (a leucineless, and a threonineless): additional sugar and virus markers were added. A mixture of the auxotrophs, or the separate auxotrophs (on methionine + Lysine) gave

When I tried again in Milan to grow the original strain on MLy, I could never get any growth out of it. Almost all derivatives of 123 were lost ~~in-the~~ when I moved from Cambridge to Milan. 123 is a poor grower; it may have been lost ^{even} at the British NCTC - at least thus told me Weigle. I am very glad to hear that Mrs. Lederberg has succeeded in doing something out of it. I have felt bitter against this strain for some time and ~~am~~ am anxious ^{to hear} if she can confirm my rather scanty experience about its self-incompatibility. I never succeeded in getting prototrophs out of it. Do they keep the original small colony type? An interesting remark about colony size is that ~~whiles~~ all ^{recombinants} ~~recombinants~~ from 123 x bm-Nfr were small-sized, there was a segregation for size when crossing 123 to Hfr.

I shall let you have as soon as written, the short paper for the local Microbiology congress, which I shall be glad to give as a joint paper with the Lederbergs. I hope you can manage reading Italian; it may amuse you, for once, to try and understand it.

Yours sincerely

Cavalli-